

Opinion. Opinion is intended to promote communication between lepidopterists resulting from the content of speculative papers. Comments, viewpoints and suggestions on any issues of lepidopterology may be included. Contributions should be as concise as possible and may include data. Reference should be limited to work basic to the topic.

Rebuttal to Murphy on Factors to the Distribution of Butterfly Color and Behavior Patterns—Selected Aspects

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In reply to Murphy's critique of my work (Jr. Res. Lep., 1985, 24:4). Since I think the scientific system ultimately decides the validity of propositions by scientific criteria, without too much attention to who holds positions pro or con, and doubt that anyone benefits from further airing of our differences, however strongly felt, on matters which I don't think have much to do with the scientific content of the book, such as why the material was published in this form rather than some other, whether the title is misleading, or why the cover is black and white, I am concerned that the most generally useful response may be none at all. (The butterfly on the cover, parenthetically, is an *Ideopsis juventa*, an oriental region danaid.) However, since Murphy proposes that he and I have basic differences about various scientific facts and scientific procedures, I will attempt to make clear my position on several points.

1. Murphy objects to the use of models or general schemes to explain observations as involving circular reasoning. I disagree, and think this is exactly the general function of models or general schemes in scientific reasoning—to bring order to uncoordinated observations or to offer an explanation of unexplained ones. It is true that making predictions is in effect proposing explanations for observations not yet made, but this is normally done in the circumstance that the proposed explanation appears to explain an observation similar to the one predicted, namely, when that model has already fulfilled its function. I cannot think of a way of explaining observations which does not require knowing what at least some are.

2. I think the definition of ecological niche given by Murphy is inadequate for butterflies because it lacks the qualifier, "... for each stage of the life cycle." I think that a definition which says that adult butterflies and their caterpillars have exactly the same ecological niches overlooks too much, and do not see the objection to the concept of niches, or subsets of

niches, for adult butterflies. The calculations I gave did not specify either the terms of such niches, nor the number of different possible values of each term, but simply illustrated what one got if one did make certain assumptions about these. Murphy and I agree that not all loci contain all possible niches, which was the point, although (see below) we disagree on why this is so, at least in part, if the interrelated features of color pattern and preferred height of flight in the vegetation are part of the definition of the "niche" of an adult butterfly.

3. Although the book contains a variety of conclusions and propositions on a variety of matters, I think the single most important part is that (Chapters 1-5 and parts of others) dealing with the proposed general scheme, which relates the color patterns of the butterfly species found at a locus to the height of vegetation at that locus and the preferred height of flight in the vegetation (from the top down) of butterflies with each specific category of color patterns. The scheme addresses the fact that, as one goes north from the tropics in this hemisphere, for example (or up a mountain in the tropics), specific color patterns disappear in a sequence, with, as one goes north, transparent patterns dropping out in Mexico, tiger-stripe (orange with transverse dark stripes) patterns at about the Texas border, and black with red patterns and black with blue patterns progressively farther north, so that at about the arctic circle (or above tree line) the only species truly resident at the locus have as color patterns only relatively "pure color" white, yellow, orange, blue or lighter brown, or intergrades of these. Murphy proposes that this is due to the reduction in total number of species resident at any locus which occurs as one goes north, but I believe this is not an adequate explanation because it does not explain the systematic shift in the proportions of species resident at any locus which have specific color patterns as one does this. The question the scheme is addressing is not, "why are there fewer transparent or tiger stripe species in the United States than there are in the tropics?," for example, but, "why are there none?"

4. The sequence given above coincides with that given by Papageorgis in her description of the layering of flight levels of butterflies with various color patterns in amazonian forests. Murphy says her paper on this is "controversial," but not that her description of the layering is incorrect, and my own field observations in five countries in the American tropics convince me that it is correct. It is presented as fact (although without specific attribution) and illustrated by Sbordoni and Forestiero (pages 212-213), for instance.

5. My proposition is that the identity of these two sequences is not an accident, but reflects the workings of a specific underlying mechanism, and I supported the proposition that the sequences are what they are because selection has "geared" color pattern to height of flight in the vegetation because each pattern is most effectively cryptic at the level in the vegetation (again, from the top down) at which species with that pat-

tern regularly fly. Murphy says that I did not adequately consider the roles of "oviposition host selection and breadth, the role of nectar as a limiting resource, the use of alternative sources of carbohydrates and amino acids, thermal constraints on butterfly activities, how resources are partitioned, how butterfly diversity and plant diversity correlate and so on." I do not see that any of these necessarily make specific butterfly color patterns occur or not occur at specific loci, and do not think his list contains the mechanism. We know for instance, both that closely related species (e.g., the viceroy and the red-spotted purple) can develop both color patterns in different color classes and the appropriately different flight levels, and that males and females of sexually dimorphic species (e.g., eastern tiger swallowtail, *Diana fritillaria*) can have patterns in different color classes. I also do not think that differences in heat-collecting capacity of different wing colors are the explanation because, for example: 1) the color pattern group most specifically associated with the deepest part of amazonian forests is the transparent one, not one of the darker ones, and; 2) the color patterns persisting in the far north are the lighter "pure color" white, yellow, orange, blue or brown ones, not the darker ones.

6. I think the next most important section of the book (Chapters 7, 8) deals with the points that a number of still stated criteria of mimicry systems are unnecessary, and in many specific instances not correct. These include the idea that in Batesian mimicry systems models must be more abundant than mimics in all loci, and that in Batesian systems models and mimics, and in Muellierian systems co-models (or co-mimics), must have the same ranges, because the rules overlooked the point that many birds migrate. Murphy happens not to criticize this portion of the book.

7. Murphy sees the data tables as a "smoke screen." Again, I disagree, because I do not think one can expect people to evaluate scientific conclusions or propositions without access to the data on which they were based. Most of the data are not mine originally, as is made clear throughout, but are derived from the publications of others, and are assuredly not generally wrong, so I think drawing conclusions from them or making propositions based on them is not scientifically inappropriate. The largest data set in the book which is strictly my own is that in the chapter on interference color patterns, which chapter Murphy happens not to criticize.

8. The book discusses a variety of other facts which "stick out" of the data, and offers conclusions or propositions based on them, including:

- a) there is, overwhelmingly, a systematic relation between the color patterns of males and females of sexually dimorphic species, and the differences follow the pattern of the classes in the general scheme. If the whole thing is chance, why should this be?
- b) the proportions of pierid and lycaenid species which have mistletoe-feeding larvae decline disproportionally as one goes north from the

tropics.

c) toxic/protected papilionids are less likely than *Papilio* species in the same regions to show sexual dimorphism with the sexes in different color classes.

d) toxic/protected species are more likely than others to have similar color patterns on both upper and lower wing surfaces.

As regards these latter two, since intraspecific Batesian mimicry is accepted for the monarch, for instance, I don't think that what amounts to propositions that intraspecific Muellerian mimicry and, in fact, intra-individual Muellerian mimicry, also occur are particularly radical ideas, but I have never heard either one presented before. To me these again illustrate the importance of access to the data. (A possible volume two, perhaps unfortunately already over 300 pages long, contains a proposed explanation of the point on mistletoes, among many other things.)